14. Robinson, H. M.: Interstitial Keratitis in Late Congenital Syphilis, South. M. J., 25:956, 1932.

15. Moore, Joseph Earle: The Modern Treatment of Syphilis. P. 330. Baltimore: Charles C. Thomas, 1933.

DISCUSSION

HAROLD F. WHALMAN, M. D. (727 West Seventh Street, Los Angeles).—I have had the opportunity of seeing most of the cases which these authors have described and have examined many of them from time to time with the slit-lamp. As a result of this personal observation, I am impressed with the importance of heroic treatment of interstitial keratitis in its acute stage.

I have watched, for the past ten years, the treatment of a large number of cases of interstitial keratitis by the usual means of chemotherapy and gained the impression, as have many others, that there seems to be very little influence of such treatment on the course of interstitial keratitis.

From a pathological standpoint, interstitial keratitis may be described in four stages.

First, the stage of infiltration, characterized by the invasion of the cornea by spirochetes, resulting in an edema of the innermost layers of the substantia propria and swelling of the endothelial cells. Nodular areas form and become necrotic, and wandering cells begin to make their appearance.

In the second stage, this avascular vulnerable tissue becomes supplied with blood vessels which grow into the deeper layers first, gradually encroaching upon the center of the cornea, and later anastomosing with conjunctival arcades which have now invaded the middle or the anterior third of the cornea. These vessels are arranged in a rather characteristic palisade all around the cornea.

The cornea is now greatly thickened and the posterior layers are beginning to show considerable sclerosis, while great radial folds are noted in Descemet's membrane. This third stage is sometimes greatly prolonged.

Finally, following the more or less complete vascularization of the cornea and accumulation of the products of inflammation resulting in the thickening of the cornea, there is a stage of restoration in which destroyed tissue is replaced by scar, the blood vessels collapse and the cornea returns to approximately normal thickness. The blood vessels remain patent and fill with blood during any subsequent irritation of the eye. These vessels are pathognomonic of a previous attack of interstitial keratitis.

From the standpoint of preservation of vision, treatment of this condition must be efficacious during the first stage, and must put a prompt end to the cellular invasion taking place. Otherwise, in the later stages of sclerosis of the cornea, such opacification will result as to seriously impair vision.

Nonspecific therapy is the only treatment that has accomplished this important step. Harvey Howard, working in the great clinic of the Rockefeller Institute at the University of Peiping, made use of intravenous typhoid inoculations for this purpose in cases of interstitial keratitis as well as other kinds of ocular inflammation. A number of authors have substantiated this work, and a few cases with which I had early experience were reported in CALIFORNIA AND WESTERN MEDICINE in December, 1931. This method, as the authors have pointed out, still has its place in the treatment of interstitial keratitis. However, their present report on the use of malaria as a nonspecific agent, indicates that its efficacy is very great and that it is a convenient and safe method of attacking a condition which may be so disastrous as to produce blindness.

I can confirm the authors' experience in receiving a negative Wassermann report at the beginning of many of these cases, so that it is important to be guided by accurate history and other clinical evidence. One such case in which vascularization was delayed until six weeks after onset, I mistook for a case of tuberculous keratitis. Tuberculin was administered without effect, and only after starting antiluetic treatment did the Wassermann return positive.

The authors are to be congratulated on presenting this important addition to the treatment of interstitial keratitis, and I am looking forward to hearing a further report from them as to the final visual results in an even larger series of cases.

WILLIAM A. BOYCE, M. D. (1210 Roosevelt Building, Los Angeles).—I have been very much interested in watching the results of the treatment of interstitial keratitis as outlined by Drs. C. Russell Anderson and Warren A. Wilson. I was fortunate in having the opportunity to see these cases before and after treatment, and the way they have responded to treatment has been most interesting and gratifying.

In the past it has been felt that every case of interstitial keratitis must run its course and that it was not influenced by constitutional treatment, and there was a certain percentage of corneas that were permanently damaged.

I consider that this treatment as outlined is a great advance in medical science, affording a ray of hope that we can pass on to our patients that something can be done to clear up these corneas rapidly and thereby give them more assurance of being able to preserve their vision.

I desire to congratulate Doctors Anderson and Wilson for their thoroughness and scientific manner in which they have attacked this problem.

THE LURE OF MEDICAL HISTORY†

TRUTH OVERTAKES "DOCTOR HUNTER"

By A. W. MEYER, M. D.
Stanford University

PART II*

AUTHOR'S DISCUSSION

SINCE, as stated above, William said that Haller credited him with the "discovery" of the independence of the fetal and maternal circulations, in 1766, it seems all the stranger that this idea was not given explicit expression in the "Gravid Uterus," which appeared eight years later. It also is noteworthy that Haller did not adopt William's idea in the third edition of his *Primæ Linæ,* in 1767, but maintained the idea expressed by him[self] in the edition of 1751, and found even in the German edition of 1788 and the English edition of 1801, in which one reads as follows: "This communication of fluids between the uterus and placenta seems to be demonstrated . . . lastly, from the passage of water, quicksilver, tallow, or wax, from the uterine arteries of the mother into the vessels of the placenta, as observed, and lately confirmed by eminent anatomists." (Pp. 437–38, Section 891.) Two eminent anatomists who thought that they had proved this, according to Fasbender, were Cowper (Bidloo's Anatomy, given out by Cowper at Oxford in 1697, in his own name) and Noortwyk (Uteri humani gravidi anatomia et historia, Lugd. Bat., 1743).[8]

In *A History of Embryology,* Needham[9] stated that Haller ". . . followed Noortwyck in asserting the separateness of the maternal and fœtal circulations in mammalia." However, as already intimated, William Hunter, Fasbender (1906) and

† A Twenty-Five Years Ago column, made up of excerpts from the official journal of the California Medical Association of twenty-five years ago, is printed in each issue of CALIFORNIA AND WESTERN MEDICINE. The column is one of the regular features of the Miscellany department, and its page number will be found on the front cover.

* Part I of this paper appeared in the issue of February, 1939, page 120.

8 California and West. Med., Vol. 45, No. 6, p. 493, col. 1, par. 1, line 2 (Dec.), 1936.

9 Needham, Joseph: A History of Embryology, p. 201. Cambridge, 1934.

Siebold[10] (1902) attributed the opposite view to Noortwyck[11] with respect to man. I regret that the original publication of Noortwyck is not accessible to me, but Fasbender quotes the following unambiguous words from it: *dubitari demum non posse de immediata communicatione vasorum uterinorum cum ovi vasis, quoniam materies, per ramum art. iliac. immissa, in vasa placentæ et chorii profunde penetraverat.*[12] (Immediate communication of the uterine vessels with the vessels of the ovum certainly cannot be doubted, inasmuch as material sent through a branch of the iliac artery penetrates deeply into the vessels of the placenta and the chorion.)

I fully realized that the above quotations from my brief articles did not and could not finally resolve the question at stake between the two brothers, but I have since come upon words written by William himself which do so with finality.

LETTERS OF WILLIAM AND JOHN HUNTER

In order to give full force to these words, it seems best to give, first, the letters written to the Royal Society by the Hunters. They are a matter of history and specifically call attention to the point at issue. It may be recalled that these letters were occasioned by John's contribution, of 1780, to that Society. William wrote to the secretary, apparently Paul Henry Maty, M.A., saying:

Windmill Street, Feb. 3, 1780.

Dr. Hunter begs the favour that the Secretary to the Royal Society will read to the Society what follows.

Mr. Hunter's account of the structure of the human placenta, explaining the connexion and circulation between the mother and fœtus in utero, which was read at the last meeting of the Royal Society, informs us that it was a discovery which he made with Dr. Mackenzie, and that it was not claimed by me. The Society will be sensible that I am reduced to the necessity of taking notice of this mistake, when they are informed of the following facts:

First. That the doctrine has been many years ago published in printed books as my discovery, and had been communicated as such by myself. See Baron Haller, for instance, in the second part of the eighth volume (p. 220) of his great Physiology in quarto, printed thirteen or fourteen years ago.

Secondly. Besides treating of it as my own discovery in my lectures on the subject, I have always done so, for many years past, in the very first lecture of my course, which is the most public of all, because the door is then open to every person whose curiosity prompts him to be present.

In the third place, occasionally in what I have printed, and in my lectures, I hope I have not overlooked opportunities of doing justice to Mr. Hunter's great merits, and of acknowledging that he had been an excellent assistant to me in this and in many other pursuits. By doing so, I always felt an inward gratification, shall I call it, or pride? I have given him all the little anatomical knowledge that I could communicate, and put him into the very best situation that I could, for becoming what this Society has, for some time, known him to be. May it be presumed then that I stand possessed of the discovery in question, till proofs shall be brought to dispossess me? I shall most willingly submit to the pleasure of the Society; if they signify an unwillingness that this emulation (shall I call it?) should go on, I shall acquiesce, and be silent. If curiosity, justice, or the laws and practice of the Society should incline the Council to seek out and determine upon the merits, I shall be equally ready to obey their commands. And if it should appear reasonable to them, I would first beg to know the grounds of Mr. Hunter's claim, as I am too well acquainted with his abilities not to think that he must be able to support his claim by something that I am ignorant of. And if I should receive that satisfaction, I shall immediately show that I am more tenacious of truth than even of anatomical discoveries. But if that information should not alter my thoughts on the question, I shall show to the satisfaction of the Society, if I can at all judge of my own employments and pursuits, that my pretensions arise out of a long series of observations and experiments made with a view to the discovery in question; that it was not a random conjecture, a lucky thought, or accidental occasion, but a persevering pursuit for twelve or thirteen years at least, the progress of which was always publicly known here, and admits of the most circumstantial proof.

WILLIAM HUNTER.[13]

* * *

John's response to William sent to the president of the same society, apparently Sir Joseph Banks, was as follows:

Jermyn Street, Feb. 17, 1780.

To the President of the Royal Society.

Sir,—Though I know the constitution of the Society over which you preside too well to suppose that they will give their judgment on any subject, and respect it too much to think it a proper field for waging the war of controversy, I cannot avoid requesting you to lay before that learned body a short answer to the paper given in by my brother, Dr. Hunter; as silence on my part, after his charge, may be interpreted by my enemies into an acknowledgment that I have intentionally claimed to myself a discovery in reality his due. I am as tenacious as he is of anatomical discovery, and, I flatter myself, as tenacious also of truth. The discovery was made in the manner in which I stated it in my paper. Dr. Mackenzie had injected the subject, and being unable, as I conceived, to explain an appearance which he had found in dissecting it, sent for me. I came to him, and having examined it further, explained the appearance in question, then, for the first time, to my own satisfaction and that of Dr. Mackenzie; and in the evening of the same day, full of the discovery, I came to Dr. Hunter, and brought him with me to Dr. Mackenzie, to see and judge of the explanation I had given and Dr. Mackenzie had agreed to. This is my state of the fact upon which I ground my belief of myself being the author of this anatomical discovery; but as my brother thinks differently, after a period of twenty-five years, I am content to abolish all remembrance of the successions of time in the course of that day, and to suppose that Dr. Mackenzie, Dr. Hunter, and myself inspected the parts together, and made the discovery, by which means the honour of it will be divided into three, one of which I may surely be allowed to take to myself, the other two may appertain to Dr. Mackenzie and Dr. Hunter, if they choose to claim, and be content with them; though in this division we must make some reserve for the claims of several ingenious young men, at that time pupils, who were with us, and of course entitled to some proportional share in the discovery, though their present situations, settled at a distance from this town, have prevented them from getting early notice of this present claim, and of course of making application to the Society for their share. However, I may here declare that if Dr. Hunter will produce to me any claim, which I can allow, of his having discovered this anatomical fact at any period of time prior to this conference at Dr. Mackenzie's, I shall first declare, in excuse for having troubled the Society, that I was not before acquainted with it, and immediately after declare that he is entitled to the sole honour of it, at least in preference to myself.

I am, Sir,

Your much obliged, and most obedient humble Servant,

JOHN HUNTER.[14]

---

10 Von Siebold, E. C. I.: Geschichte der Geburtshülfe, Zwei Bände. Tübingen, 1901.

11 Noortwyck, W.: Uteri humani gravidi anatomia et historia. Lugd. Bat., 1743.

12 Fasbender, Heinrich: Geschichte der Geburtshülfe, p. 414. Jena, 1906.

13 Paget, Stephen: John Hunter, Man of Science and Surgeon (1728–1793). With introduction by Sir James Paget, pp. 70–73. London, 1897.

14 *Ibid.*, pp. 73, 74.

COMMENT ON THE LETTERS

In regard to the first point in William's letter, it should be noted (1) that he mentioned no book in which his discovery was acknowledged except that of Haller, which contains no such acknowledgment, but refers instead to the page in William's *Medical Commentaries* containing a footnote in which the contrary opinion is vigorously defended; and (2) that he failed to say to whom he communicated his discovery unless he meant the public spoken of in "Secondly." This recalls to mind the comment of Paget, in the case of the quarrel between the Hunters and Pott, that William had recourse to rhetoric while John gave "a plain statement of all that he remembered."

Department of Anatomy.

(To be continued)

DEATH OF "DR. O. UPLAVICI"

By W. H. MANWARING
Stanford University

IN 1887 Dr. Jaroslav Hlava of Czechoslovakia reported the discovery of amebas in the stools and intestinal ulcers of patients suffering from dysentery, together with his success in transferring the disease to laboratory animals (cats) by intrarectal inoculation with human ameba-containing stools. His paper was published in the leading Czech medical journal of that day, under the title: "O uplavici; Predbezne sdeleni." ("On dysentery; a preliminary communication.")

By some unexplainable editorial oversight, Dr. Hlava's name was omitted in the German reviews of his paper. Credit for this basic medical discovery was, therefore, given to "Uplavici, O" (Dysentery, On).[1]

For fifty years international medical science paid homage to the mythical bacteriologist, "Dr. O. Uplavici," ranking him with Pasteur, Koch, and Lister, as one of the outstanding pioneers in modern medical science. It was not until a year ago that the mythical nature of this nineteenth century protozoölogist was recognized by Dr. Clifford Dobell of London, England, and a formal obituary of "Dr. O. Uplavici (1887-1938)" published in a leading English medical journal.[2]

While the creation and perpetuation of the "Uplavici" myth has probably neither hastened nor retarded the development of bacteriological science, Doctor Dobell's obituary may have a salutary effect on future medical historians. There are a number of other minor literary myths still honored in medical research literature, most of them of fairly recent Russian or oriental origin.

P. O. Box 51.

1 Centralbl. f. Bakt., 1: 537, 1887.
2 Dobell, Clifford: Parasitology, 30: 239 (June), 1938.

Seldom shall we see in cities, courts, and rich families, where men live plentifully and eat and drink freely, that perfect health and athletic soundness and vigor of constitution which are commonly seen in the country, where nature is the cook and the necessity the caterer, and where they have no other doctor but the sun and fresh air.—South.

CLINICAL NOTES AND CASE REPORTS

CEREBRAL EDEMA

INJECTION OF AIR INTO THE SPINAL CANAL AS A THERAPEUTIC MEASURE

By GEORGE H. SCIARONI, M.D.
AND
K. F. SHARP, M.D.
Fresno

PREVIOUS reports on the value of spinal puncture as a therapeutic measure in the treatment of cerebral edema have confined themselves to a discussion of the procedure as a means of decreasing intraspinal pressure by withdrawing small quantities of spinal fluid at certain intervals. This may or may not give the patient some relief from the distressing symptoms of increased intracranial pressure. We know that many cases of cerebral edema fail to show any appreciable increase in the pressure of the spinal fluid. This is particularly noticeable in some cases of epidemic encephalitis and brain concussion. Many of the varieties of encephalitis may fail to show increase in the pressure of the spinal fluid, as we have had occasion to observe in the patients we have seen in the San Joaquin Valley.

The usual method of treatment of these patients has limited itself, to some extent, to the use of hypertonic solutions of glucose, sucrose, magnesium sulphate, etc., intravenously. One of us, Dr. G. H. Sciaroni, has been using air injections into the spinal canal for a number of years as an important addition to the intravenous use of hypertonic solutions. The procedure he has used, and that we will describe here, demonstrates its value, not by decreasing spinal fluid pressure, but by deliberately increasing pressure within the cranial cavity. The patient is usually seated in an upright position, and a spinal puncture is done. No fluid is removed, but a small quantity of air, usually 20 to 50 cubic centimeters, is injected into the spinal canal, and this air rises to the cranial and ventricular cavities, where it exerts pressure on the brain, compressing the tissue and forcing the fluid out of the brain and into the blood vessels. Hypertonic glucose solution, 50 cubic centimeters of 50 per cent glucose, is usually given intravenously just preceding the spinal puncture, and we have noticed some unusually good results. Many of the patients treated in this manner were so seriously ill before air injections were begun that their prognosis was almost hopeless, and we have seen these patients make rapid and complete recoveries, showing improvement almost at once after air injection into the spinal canal was begun.

As we have mentioned, this form of treatment has been used for some time by Doctor Sciaroni, particularly during the spidemics of encephalitis in 1933 and 1934. His results were exceptionally good. We are reporting this more recent case of encephalitis because of many unusual features, the most remarkable of which was the rapid improvement shown by the patient as soon as air injections